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Referral and Job Performance: Evidence from the Ghana Colonial Army

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I. Introduction

Since Granovetter's (1974) seminal work, it is widely recognized that job referral plays an important role in the way the labor market works. There are many different types of referrals (e.g., by relatives, teachers, or previous employers). One kind of referral that has attracted the attention of economists is referral by current employees. This form of referral is thought to play three possible roles: the transmission of information that is relevant to the hiring process, the monitoring of workers after recruitment, and the reduction in search costs when attracting suitable workers is difficult. In these cases, referral enhances efficiency either by increasing effort and productivity through employee monitoring (Kugler 2003; Bandiera, Barankay, and Rasul 2005; Heath 2011) or by raising the quality of the match, either by providing employers with better information about workers (Saloner 1985) or by providing workers with better information about job characteristics (Simon and Warner 1992; Mortensen and Vishwanath 1994). Referral can also be an exchange of favors between employer, referee, and new recruit. In this case, referral is a likely source of inefficiency and inequity since it distorts the

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recruitment process (e.g., to favor friends and relatives; Barr and Oduro 2002; Bandiera, Barankay, and Rasul 2009).[1]

Montgomery (1991) provides an elegant formalization of screening through employee referral. In his model, referral by employees is valuable because the unobserved quality of a new worker is positively correlated with the revealed quality of the current employee providing the reference. If the current employee has proved to be of high quality, anyone referred by this employee is also more likely to be of high quality. Underlying this assumption is the idea that social ties are characterized by homophyly, and hence that characteristics of socially proximate individuals are correlated (Jackson 2008). As Montgomery shows, this assumption is sufficient to induce employers to rely on referral from high-quality employees. It does, however, assume that referees truthfully report the information at their disposal. Whether this is the case in practice is unclear.

We test whether employee referral helps identify workers with better unobserved characteristics at the time of hiring. We do this using recruitment data from the British colonial army in Ghana, 1908–23. The conditions under which the Gold Coast Regiment (GCR) operated provide an excellent vantage point from which to study employee referrals. The GCR was a big employer, with a peacetime strength of about 1,500 rank and file drawn exclusively from the indigenous population. Although there was a large army buildup during World War I, universal conscription was never introduced, and the GCR had to compete for labor on the labor market.

Until 1923, the army explicitly used referrals by fellow soldiers. Depending on the year, 20%–80% of new enlistees were brought in by fellow soldiers, often from their home village or region. In 1908–18 a financial reward was paid to the referee, and the army kept a record, which means that there is clear and unambiguous information on employee referral. Another welcome feature is the homogeneous work conditions and skill requirements—compensation was uniform for new recruits, and the tasks assigned to them were fairly basic and relatively similar. We thus have a large number of observations relative to a single employer seeking many workers with similar characteristics and using employee referral in a systematic way.

Since the colonial army remunerated servicemen who brought suitable new recruits, we expect referral to be beneficial to the army as an employer. Given the context, better monitoring is unlikely to have been the main motive. This leaves two likely candidates: identifying recruits with better unobserved char-

[1] Goldberg (1982) argues that less competitive industries can maximize utility rather than profits, and this allows them to rely on nepotism. Since we only have data from one employer, we cannot test Goldberg's conjecture.

acteristics or reduction in recruitment and search costs. In the first case, we expect referred recruits to have better hidden characteristics than unreferred ones; in the second case, this need not be true. This observation forms the basis of our testing strategy.

One key characteristic for army work is physical strength, a trait that is correlated with height. Other relevant characteristics include loyalty and discipline. Height is observable at recruitment; loyalty and discipline are only revealed later. If referees bring in better soldiers, new recruits referred by servicemen should over time be revealed to have better unobserved characteristics. The Ghanaian colonial army offers a convenient vantage point into this issue. Unlike other employment contracts, army recruits cannot leave of their own accord. If they do, it is recorded as desertion and can be prosecuted. Employee dismissal is also under the control of the employer, who customarily records the reason for termination. We therefore have a clear record of employees' revealed performance as seen by the employer.

Using army records covering enlistments over 1908–23, we compare the initial physical ability and subsequent performance of referred and unreferred recruits. We also test whether referred recruits brought in by higher-rank servicemen are better than those brought by unpromoted soldiers: if the hidden characteristics of socially proximate individuals are correlated, then better workers should bring in better recruits. This assumes that referees report information truthfully. They may however behave opportunistically, either to obtain the reward or to ward off pressure from superiors to bring in new recruits. Referee opportunism would lower the unobserved quality of new recruits, perhaps even below that of unreferred recruits. If this is the case, then the army must derive some other benefit from employee referral (e.g., reduction in recruitment cost) for the practice to be encouraged and remunerated.

To deter opportunistic referral, the army may penalize soldiers who refer low-quality recruits, for instance, by reducing their chance of promotion. We do not observe this directly, but, if this were the case, soldiers who have no more promotion prospects would be harder to penalize for bringing in worse recruits. Consequently, if referee opportunism is present and the army penalizes bad referees by lowering promotion chances, we expect that high-rank soldiers should refer recruits of lower unobserved quality than low-rank referees.

Results strongly reject the idea that, in the case of the Ghanaian colonial army, employee referral improved the unobserved quality of recruits. While referred recruits were usually taller than unreferred ones, they were also more likely to desert or be dismissed as inefficient or unfit. Furthermore, recruits referred by higher-ranked servicemen were of lower quality than those referred

by low-rank soldiers. These results survive a battery of robustness checks to which we subject them. Our findings are consistent with referee opportunism, and they may explain why after World War I the colonial army stopped rewarding servicemen for bringing in new recruits.

These results contribute to the literature in several ways. First, much of the currently available evidence on employee referral comes from developed economies with large and active labor markets. Little evidence relates to other parts of the world, either now or in the past. We offer evidence from Africa during the colonial period. Our findings are different, suggesting that results obtained in one context may not be applicable elsewhere.

In developed economies, referred workers have often been shown to earn higher wages, have higher productivity, and enjoy lower turnover and higher tenure than other workers (Corcoran, Datcher, and Duncan 1980; Datcher 1983; Korenman and Turner 1996; Holzer 1997; Kugler 2003).[2] Similar results were found in Egypt by Antoninis (2006), who reports a positive correlation between the start-up wage in an manufacturing firm and referral by a former colleague. Such findings have been interpreted as evidence of better match quality for referred workers. However, they suffer from potential sources of bias, such as survivor bias and variation in skill levels by recruitment channel. They could also be manifestations of employer nepotism, as, for instance, suggested by Barr and Oduro (2002). Our test is not subject to the same ambiguity because all new indigenous recruits in the colonial army were given identical pay and contract conditions. Unlike a start-up wage, the measures of performance that we use cannot be suspected of reflecting favoritism: deserters were hunted down and jailed (or even shot), and their deferred salary was not paid—hardly an expression of favoritism by employers.

That truth-telling need not be incentive compatible has long been recognized by the economic literature on contracts.[3] We find that a similar problem may affect referral by employees. Our context and findings are in line with recent experimental evidence gathered by Beaman and Magruder (2012) in modern day India. The authors asked experimental participants to refer people for a task. Referees were paid either a fixed amount per referral or an amount

[2] Different findings are reported by Bentolila, Michelacci, and Suarez (2010). Using data from the United States and European Union, the authors find that referrals through family, friends, or other contacts (not necessarily of those whose productivity is known to the employer) lower start-up wages but also the length of the unemployment spell. They interpret their results as suggesting that referral is used to shorten a search, with the downside of lowering match quality, reflected in lower wages.

[3] Fafchamps (2004), e.g., presents evidence that there is little information sharing among African firms, making collective punishment for breach of contract difficult if not impossible. The explanation he offers is that entrepreneurs seldom regard the provided information as reliable, unless they know and trust the source of the information.

contingent on the performance of the referred person. Beaman and Magruder (2012) find that when payment for referral is contingent on performance, participants are less likely to refer a relative and more likely to refer a high performer. These findings suggest that, unless truth-telling is incentivized, referees are at least partly motivated by the exchange of favors with friends and relatives or some other form of referee opportunism. The authors also find that the effect of the incentive on the quality of referrals depends on the quality of the referee: participants of low ability show little capacity to recruit high-performing referrals. This suggests that high-ability referees are able to identify high-ability referrals as assumed by Montgomery (1991), but need not do so unless they have the right incentive. We come to similar conclusions by comparing the unobserved quality of recruits referred by soldiers who can and cannot easily be penalized for bringing a poor-quality recruit. We find that referee quality is lower when referees are harder to penalize.

The article is organized as follows. Background information on the GCR is provided in Section II, and the data are discussed in Section III. Our testing strategy is outlined in Section IV, and empirical results are presented in Section V.

II. Background

The GCR was a big supraregional employer, with a peacetime strength of about 1,500 men.[4] Its primary role was to maintain internal security, safeguarding against unrest and uprisings such as the one in Ashanti in 1901. Rarely, the GCR was involved in punitive expeditions.

All commissioned officers in the GCR were British, but the entire rank and file was drawn from the indigenous population, with a significant share of recruits (20%–40%) originating from neighboring colonies. Universal conscription was never introduced, and the GCR had to compete for labor on the labor market, particularly with mining and cocoa farming and other forms of employment, most importantly own-account farming. Military service at that time involved unskilled labor with little degree of differentiation. The ideal recruit was a man who was loyal and amenable to discipline. All new recruits received the same pay.

Various recruitment methods were used. First, recruiting parties toured an area encouraging men to enlist. Second, chiefs who, under the system of "indirect rule" acted as agents of the colonial administration, were asked to provide recruits using financial inducement and coercion. Third, until 1923 soldiers of the GCR were explicitly encouraged to bring new recruits, often from

[4] This section draws heavily on the excellent work of Killingray (1982).

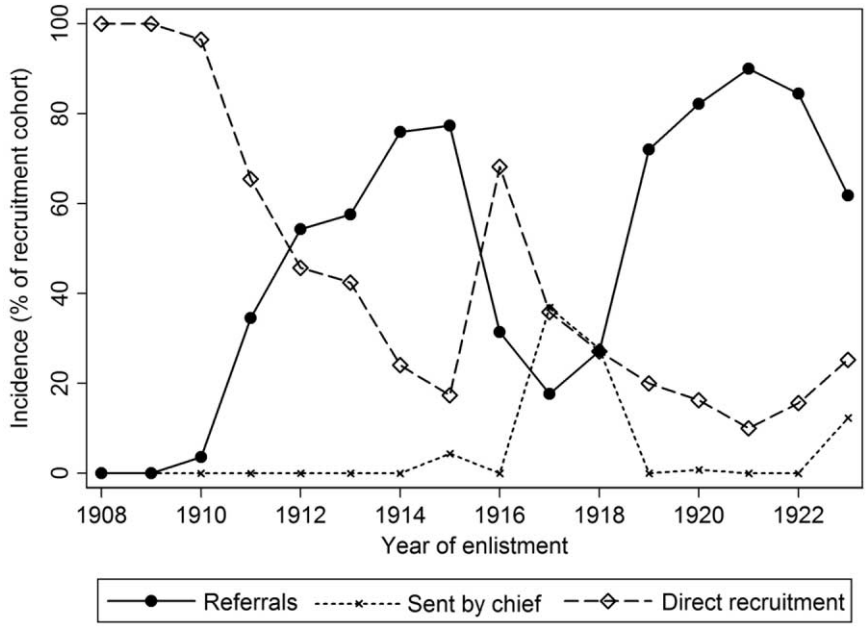

Figure 1. Recruitment channels by year of enlistment

their home village or region. From 1908 until the end of World War I, a practice of "bringing in money" was systematically applied: upon acceptance of a new recruit, the bringer received a payment equivalent to about a week's pay.[5] The recruitment channels by year of enlistment are summarized in figure 1.

Recruits had to pass certain minimum requirements laid down in official army handbooks, although officers had some discretion in enforcing these rules. Among the easily observable requirements were height and chest circumference, which were used to screen the health and physical fitness of potential recruits. The standard contractual length of service was 6 years with the colors plus 3 years on the reserve. In late 1916 an additional category became commonplace: recruits could choose to serve until the war ended. In our sample, 34.6% and 57.3% of the recruits had "6 + 3" and "duration of war" recorded as terms of enlistment on their attestation papers, respectively.[6] Extensions were possible but required the approval of the army.

The GCR maintained several bases of varying size across Ghana. The two main bases were Kumasi and Accra, where 65% of the recruits in our sample

[5] Killingray (1982), 273, mentions an amount "varying from five shillings to ten shillings" (referring to 1896).

[6] Residual categories were 2 years (6.4% of recruits), 12 years (0.9%), and 3 years (0.8%). Results are robust when limiting our analysis to the "6 + 3" and "duration of war" categories only.

were attested and where the practice of referrals was equally common.[7] Referrals were widely practiced across the country, albeit with some variation (e.g., they were used more often in some of the peripheral and poorer areas of the north).

Referee and referred were unlikely to work together. About 10% of the referees were ex-GCR members serving, at the time of referral, in the organizationally separate paramilitary police force in northern Ghana and in the reserve. New recruits joined a training unit for the first 6 months of their service, after which they were transferred to regular units.[8] There is no evidence that referred recruits were treated differently in placement. In our data set we have at least five battalions and 16 companies. Daily contact and direct oversight is likely to have taken place at the platoon level. Under random placement, the likelihood that the referee and referred were in the same company or platoon is about 6% and less than 1%, respectively.

Once the recruit was accepted and had sworn his oath, he was subject to military law. This meant that punishments for breaches of discipline could be imposed, including fines, drill, general fatigue work, detention, and imprisonment (WAFF 1923). Moreover, the army withheld about one-third of the soldier's basic pay as compulsory saving until completion of service. This policy acted as deterrent against desertion.

Local recruits all had to earn promotion through the ranks, on the basis of merit and ability. Africans were not allowed to hold positions of responsibility that would place them over white army men. Hence, the highest rank that Africans could reach after long and distinguished service was regimental sergeant major (RSM). Promotion, however, paid off. The basic daily rate of pay for a private was 1 shilling. This increased to 1.25, 1.5, and 3 shillings for a corporal, sergeant, and company sergeant major (CSM), respectively. In addition, the military offered living allowances and occasional gratuities and rewards; nonmonetary benefits included uniforms, housing, and medical care.

World War I dramatically increased the demand for recruits as GCR troops were used in Togoland (1914), the Cameroons (1914–16), and especially the east African campaign (1917–18). About 7,000 men enlisted in the GCR during the war, compared to a yearly intake of about 200 recruits in peacetime. The years 1917 and 1918 stand out, with 3,800 and 1,600 new recruits, respectively. These numbers were obtained along the recruiting channels mentioned above: recruiting parties were intensified, and chiefs had to fulfill quotas. Recruitment was extended to ethnic groups and areas previously not—

[7] For summary statistics and maps, see online app. A.

[8] Only 15 referrals came from a referee in the training unit.

or less—targeted such as Ashanti and the coastal peoples of Ghana and Togo. Physical requirements were also reduced. Traveling by sea to fight in east Africa was extremely unpopular. Consequently, new recruits were guarded and quickly shipped to east Africa before even completing their training. Corporal punishment (flogging), abolished in 1908, was reintroduced in 1917 to prevent absenteeism and desertion.

Outside options were a likely cause of desertion. They varied over time. For example, desertions of soldiers who were farmers before enlistment follow a seasonal pattern with rates twice as high in May–July (cocoa harvest in southern Ghana) and October/November (harvest in the north). During World War I the labor market was suppressed generally. Cocoa production was less dynamic during the war, and cocoa prices fell due to lower demand; not all cocoa pods were collected, and production decreased in 1917. During World War I, miners were sacked because of a lack of explosives. Literate soldiers who left the army legally could find employment as clerks and command a higher wage than illiterate farmers in the cocoa economy. Working as a clerk would presumably have been more difficult for a deserter.

III. Data

The data used in this study were collected from military personnel records held by the General Headquarters of the Ghana Armed Forces in Accra, Ghana. We drew a complete sample of recruits enlisted between 1912 and 1923. We had more difficulty locating recruits from 1908 to 1911, who are somewhat underrepresented. Overall, the raw data set consists of 7,616 fighting troops and 844 motor drivers.

The army collected a wealth of information on each recruit, including his age, place of birth, ethnicity, and previous occupation. Additionally, height and chest circumference were measured as part of a routine medical examination. Summary statistics are given in table 1. The circumstances by which the recruit entered the army were recorded on attestation papers. Entries in the field "Bringer" fall into three categories: (*a*) referrals by a fellow soldier stating his name, rank, and regimental number; (*b*) recruits sent by chiefs; and (*c*) volunteers. Around 34% of soldiers were recruited through a bringer, another 23% were sent by chiefs, and 8% are recorded as having volunteered. The rest were recruited directly by the army.[9] We identified 1,127 bringers, who account for 2,837 referrals. The majority took on the role of

[9] We suspect that "volunteers" is not a distinct recruitment category but were hired directly by the army as well.

TABLE 1
SUMMARY STATISTICS

Variable	Observations	Mean	SD	Min	Max
Characteristics at time of recruitment:					
Height (cm)	8,295	168.1	7.3	125.73	203.2
Chest circumference (cm)	8,134	85.4	5.3	63.5	139.7
Age (years)	8,269	24.2	4.9	14	55
Recruiting channel:					
Referral	8,460	34.2%			
Sent by chief	8,460	22.8%			
Volunteered	8,460	7.9%			
Direct recruitment by army	8,460	35.0%			
Deserted	8,460	11.1%			
Dismissed as medically unfit/inefficient/for misconduct	8,460	16.3%			
Bringer (yes = 1)	8,460	3.4%			
Rank of bringer at the time of referral:*					
Low rank:					
Private	8,460	9.2%			
Lance corporal	8,460	3.2%			
Corporal	8,460	3.2%			
High rank:					
Sergeant	8,460	6.4%			
Company sergeant major and regimental sergeant major	8,460	6.8%			
Unknown and other (*malam*, pay clerk, etc.)	8,460	5.4%			
Bringer is kin	8,460	2.9%			
Recruit has a relative in the army	8,460	15.3%			
Previous occupation:					
Farmer	7,923	65.0%			
Soldier/police	7,923	2.0%			
Literate (yes = 1)	8,460	6.7%			
Skills in previous occupation (1 unskilled to 5 skilled):					
Unskilled (laborer, carrier, boy, etc.)	7,923	13.4%			
Semiskilled (cook, steward, sawyer, fisherman, etc.)	7,923	7.1%			
Skilled (carpenter, tailor, goldsmith, etc.)	7,923	8.9%			
Semiprofessional (clerk, trader, schoolboy)	7,923	4.6%			
Professional (engineer, teacher, etc.)	7,923	.9%			
Conditions of employment:					
Distance between place of birth and enlistment (in 100 km)†	3,860	1.83	1.71	0	6.6
Motor transport unit	8,460	.10			
Enlisted in World War I	8,460	.81			
Enlisted in 1917–18	8,460	.64			
Military conditions at time of recruitment:					
Desertion rate in the 12 months before enlistment	8,460	7.4	2.3	0	18.2
Dismissal rate in the 12 months before enlistment	8,460	6.7	6.9	0	48.9
Time at risk 12 months before enlistment (soldier years)	8,460	1,783.8	1,371.3	1	5,151.6

* Calculated over all recruits; proportions sum to 34%.
† For Ghanaian recruits only

TABLE 2
RECRUITING CHANNELS AND CAUSE OF DISCHARGE (ENLISTMENTS, 1908–23)

	Recruiting Channel									
	Referral		Sent by Chief		Volunteered		Direct Recruitment		Total	
Cause of Discharge	Number	%	Number	%	Number	%	Number	%	Number	%
Deserted	419	14	234	12	55	8	229	8	937	11
Dismissed as inefficient	231	8	55	3	16	2	79	3	381	5
Dismissed as medically unfit	318	11	254	13	41	6	277	9	890	11
Dismissed for misconduct	57	2	20	1	6	1	26	1	109	1
Other	1,870	65	1,370	71	553	82	2,350	79	6,143	73
Total	2,895		1,933		671		2,961		8,460	

Note. "Other" summarizes causes of discharge that indicate a soldier of good quality such as "cessation of hostilities" (18.0%), "completion of service" (8.1%), "died" (3.4%), and "killed in action" (.8%). For 42.4% of the soldiers, the cause of discharge was not entered on the attestation paper but is most likely related to a regular termination of contract.

bringer on only one occasion; 96% of bringers referring less than 10 recruits account for 70% of all referrals. A small number of (long-serving) soldiers were active recruiters: 46 bringers in varying positions ranging from *malam* (a Muslim "military chaplain"), pay clerk, and private to RSM account for referrals of the remaining 928 recruits. We know from secondary sources that the bringer was compensated; the actual compensation amount was unfortunately not recorded on attestation papers, which are our primary source of information.

Personnel files contain information on the soldier's conduct and career within the army. The cause of discharge is an excellent indicator of a soldier's quality from the point of view of the army. "Deserted," "inefficient," "medically unfit," and "misconduct" are categories that indicate poor performance as seen by the employer, whereas the remaining categories such as "demobilization" or "completion of service" indicate good performance. The cross-tabulation of frequencies is presented in table 2. It is clearly evident that the GCR as an employer had massive problems recruiting reliable, physically fit, capable men: one-fourth of enlisted recruits did not meet these requirements ex post (i.e., they deserted or were discharged as "medically unfit" or "inefficient" or for "misconduct"). Casualty rates were low in the African campaigns (Killingray 1978).[10] In our analysis, we treated these men as good quality soldiers.

A substantial proportion (38%) of the attestation papers are silent on termination. After the war, the army modified its system for recording details of

[10] In our sample, 0.8% and 3.4% of the soldiers were recorded as "killed in action" and to have "died," respectively. At 1.5% and 4.0%, rates were slightly higher among referred recruits.

discharge. It appears that information is missing for recruits who completed their service normally. Only those who deserted or were dismissed early were recorded on attestation papers.[11] In our analysis, we assume that if the date of discharge is missing on attestation papers, the soldier served at least until the end of World War I and demobilized at some unknown later date.[12] The proportion of desertions and dismissals is summarized in figure 2 by year of enlistment cohort.

IV. Testing Strategy

There may be multiple reasons why employers recruit individuals referred by their workers. Here we focus on screening and saving on recruitment costs, which are the most likely candidates given the context. Monitoring is unlikely to have been a reason, given that referee and referred served in different units and hence were hardly ever in a position to monitor each other. Since other possible motives cannot be tested with the data at hand, we focus the presentation of our testing strategy on what we can empirically investigate. A detailed discussion of identification issues can be found in the appendix. We focus here on the main intuition behind our testing strategy.

Let q stand for the quality of a new recruit, and let $R = 1$ if the recruit is referred by another soldier, and 0 otherwise. Recruit quality q can be divided into components h that are observable at the time of hiring (such as height) and unobservable components d (such as loyalty and sense of discipline). Let $q = f(h, d)$.

A. Screening

If worker referral serves a screening purpose, it helps employers select new recruits with better unobserved characteristics. In this case we expect that referred recruits prove to be more loyal and disciplined:[13]

[11] The numbers of deserters in our data agree with figures reported by the governor to the secretary of state (Thomas 1975). Individual entries of desertions and dismissals also match the 1914–15 Star Medal Roll of the GCR. The roll lists the name and regimental numbers of deserters and others who were not eligible to the medal, which was awarded to all members of the GCR who were deployed in Togo and Cameroon between August 5, 1914, and December, 31, 1915. Source: Service Medal Rolls WO 329/2956, The National Archives, Kew.

[12] This brings the number of soldiers (4,653 fighting troops and 693 motor drivers) serving at the end of World War I into line with estimates by Killingray (1982).

[13] Referral could be a bad signal to the employer, for instance, because, as in Montgomery (1991), the employer knows the referee to be of bad quality. In this case the employer would turn a referred worker down—or offer worse employment conditions. Anticipating this, a job candidate could simply volunteer instead of being referred, something that was always possible during the study period. It follows that recruits who are recorded as referred should, in a screening model, always have better unobserved quality.

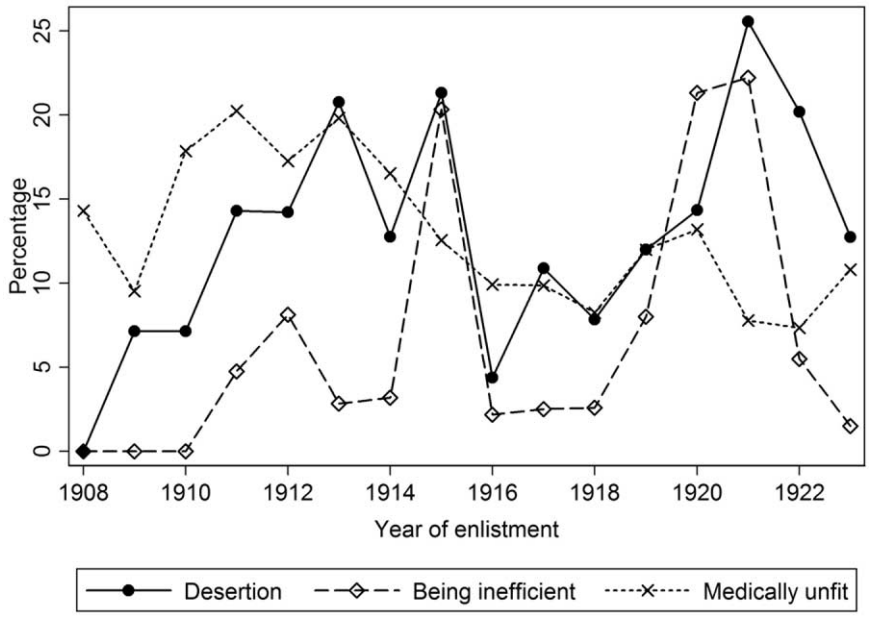

Figure 2. Desertions and dismissals by year of enlistment (cohort effects)

$$E[d \mid h,\ R = 1] > E[d \mid h,\ R = 0]. \tag{1}$$

Screening models make less clear predictions about observable characteristics h. Montgomery (1991) assumes that the characteristics of referee and referred are correlated in general. If this holds for both observables and unobservables, we expect:[14]

$$E[h \mid R = 1] > E[h \mid R = 0]. \tag{2}$$

Referral can serve a useful screening purpose for the employer even if (2) is violated. For instance, if the employer expects referred workers to have a higher d, it may be optimal to hire them even if they have a lower h. The important point is that a pure-screening model of referral would never predict a situation in which referred recruits are better in terms of observable h but worse in terms of unobservable d. Indeed, in this case referral would not provide any valuable information since h is observable at the time of hiring. This simple observation forms the basis of our testing strategy: if $E[d \mid h,$

[14] The maximum reward that the employer is willing to pay for a referral is $E[q \mid R = 1] - E[q \mid R = 0]$. If the reward necessary to incentivize the bringer is smaller than this maximum, referral is optimal for the employer and should be observed in the data.

$R = 1] < E[d \mid h, R = 0]$, then screening on unobservables cannot be the motive for using employee referral.

B. Saving on Recruitment Costs

Equation (1) need not hold if worker referral simply economizes on recruitment costs. An employer may optimally recruit a referred worker expected to be of inferior quality if the cost savings exceeds the reduction in the value of the recruit to the employer. It is possible for employers to expect the unobservable quality of referred recruits to be worse than unreferred ones. But if they do, they are more likely to insist on higher observables h such that (2) holds.

Height and chest circumference recorded at the time of recruitment are taken as measures for h. Adult height is a widely used and viable indicator of nutritional and health status (WHO 1995). Regarding chest circumference, a positive correlation has been found with lung capacity, body mass, and longevity. But the evidence remains contradictory, and the medical literature nowadays largely disregards chest circumference as a predictor of fitness (Yao et al. 1991; Wu et al. 2009). In early twentieth-century army recruitment, however, chest circumference was, together with height, considered an important and reliable indicator of physical fitness (Pignet 1901), and recruits had to meet a minimum requirement. Similar principles were applied elsewhere in British colonies, and chest circumference is also a good predictor of promotion in the Kenya colonial army (Moradi and Mylavarapu 2008).

For d we use information on contractual performance as indicated by the cause for termination recorded in the personnel file (e.g., whether desertion, inefficiency, medically unfit, or misconduct is listed as cause of discharge). One may argue that recruits with better outside options were more likely to desert. Worker productivity, however, is firm specific. From the perspective of the army, deserters undo firm specific investments and, more importantly, undermine morale and put military operations at risk—and hence were clearly not desired.

C. Control Variables

Equation (1) represents a correlation, not a causal relationship. Hence, we avoid many of the complications induced by the difficulty of correctly identifying causal effects. But to reject screening, we need to compare workers who are observationally identical at the time of recruiting and show that the referred worker is differentially likely to desert or be dismissed. In other words, we must control for characteristics of new recruits that are observable to the employer. Failure to do so would lead to incorrect inference if the omitted

characteristic is correlated with *R*—see the appendix on identification for a formal discussion.

We also need to control for time-varying factors, such as the onset of World War I, which worsen employment conditions and affect the potential recruitment pool—and thus raise the likelihood of poor performance (e.g., desertion)—and which may be correlated with the use of referral as a recruitment method. Similarly, referrals may be used to reach more remote populations and more "amenable" communities, a confound that we deal with by including ethnic group fixed effects.[15] Similar arguments can be made for literacy, previous occupation, skills, and age of recruits—which were observed by the army at the time of recruitment, may be predictors of unobserved quality, and may be correlated with recruitment by referral. Farmers, for instance, were most likely to desert, whereas literate recruits were not. We therefore control for all characteristics that were recorded in writing at the time of recruitment. The army deemed this information to be important for the recruitment process, which is why its collection was institutionalized.

D. Opportunistic Behavior

So far we have assumed that referees do not behave in an opportunistic manner and truthfully pass onto their employer the information at their disposal. In practice, referees may misrepresent the information they have—or claim to have. For instance, a soldier may recommend favorably someone he does not know in order to get the reward. Another possibility is that the army puts pressure on soldiers to bring new recruits, as seems to have happened sometimes, according to Killingray (1982). If this is true, we expect bringers to put little effort into identifying high-quality recruits. They may also misrepresent army life to new recruits in order to lure them into applying. Alternatively, the referee may recommend a friend or relative he knows to be poorly qualified for army work. Although these examples differ in terms of intent, we categorize them all under the umbrella of "opportunistic referral."

The army could in principle deter opportunistic referral by making the bringer's reward contingent on the recruit's revealed quality. Historical records do not show evidence of such practice. However, we cannot rule out the possibility that the army penalized opportunistic bringers in terms of subsequent promotion. This, however, is a weak incentive for higher-ranked referees who already reached the top of the career ladder.

[15] During the study period, ethnicity is a very disaggregate geographic, economic, and community marker. In our data set we have 81 ethnic groups, compared to 31 administrative regions at that time.

Using referral as a screening device may still be optimal for the employer in spite of opportunism, as long as $E[d \mid h,\ R = 1] - E[d \mid h,\ R = 0]$ remains positive. But the more opportunistically referees behave, the lower the unobserved quality of referred recruits, something that army recruiters may seek to compensate for by insisting on higher observed characteristics h. We would then observe referred recruits to be taller at the time of hiring but over time to reveal themselves to be less disciplined. If, as is likely, the army correctly anticipates this, referral only makes sense if it economizes on recruitment costs.

E. Desertion, Deterrence, and Referrals

As argued by Costa and Kahn (2003), desertion is a strong indicator of recruit quality from the point of view of the army. Desertion is punishable, and the punishment acts as a deterrent to desertion. Not every deserter was apprehended and punished, however.[16] We expect the risk of apprehension to vary with distance between place of birth and army base. In close-by villages the army can better verify information provided on recruitment that later can lead to the apprehension of deserters, such as the village of birth and the address of relatives. In the case of recruiting parties, for example, this happens on site. Moreover, the army might lack local knowledge and find it too costly to search for deserters in distant places.

Deterrence varied over time. During the war—especially in 1917–18—the penalties for desertion were significantly increased. These increased penalties nevertheless have less deterrent effect on those runaway soldiers who can avoid being found. This suggests a strategy for testing the deterrent effect of distance. Let a denote punishment inflicted on a deserter, conditional on being found. Let f denote the probability of being found, which we assume to fall with distance k between the recruit's place of birth and his military base. Expected punishment is $af(k)$. The probability of desertion p is a decreasing function of $af(k)$. Now consider an increase in the penalty Δa. How does this increase affect deterrence as a function of distance? We have

$$\frac{\partial(\Delta af(k))}{\partial k} = \Delta af'(k) < 0,$$

which means that an increase in the penalty has a stronger deterrent effect on recruits posted near their place of origin. If we let $f(k) = b - fk$, the above

[16] A reward was paid for information that led to the apprehension of deserters. The rate of apprehension and conviction varied. In 1906, for example, 24 of 40 deserters were apprehended, whereas 7 of 50 deserters were apprehended in 1907. Report of the Inspector General, CO 445/24, The National Archives, Kew.

boils down to an interaction term between distance k and the change in penalty Δa. We expect the coefficient of $k\Delta a$ to imply that the relationship between distance and desertion increased with stricter penalties.

The risk of apprehension is likely to follow a different model for referred soldiers, because the referee can verify information and help to locate the runaway soldier irrespective of distance. The likelihood of punishment is thus higher. This generates a confounding effect—a reason other than a higher d to expect referred recruits to be less likely to desert. Consequently, other things being equal, we expect a lower risk of desertion from referred recruits. Of course, if referred recruits are less disciplined, they may be more likely to desert in spite of the penalty. But this does not mean that the deterrence effect of referral is absent. To investigate this possibility, we interact $k\Delta a$ with a referral dummy: if a bringer can help locate a deserter irrespective of distance, the coefficient of this triple interaction term should be equal to minus the coefficient of $k\Delta a$. We test this prediction in the data. The same argument does not apply to dismissals, a prediction that we also test.

F. Chiefs

We focus the analysis on referral by fellow soldiers because it was remunerated and is driven by individual motives. Referral by chiefs is subject to some of the same issues as referral by soldiers: the army may have sought to reduce recruitment costs, or it may have expected referred recruits to be of higher quality because, through their networks, chiefs have access to information about recruit quality.

Referral by chiefs is also subject to opportunism. Chiefs were pressured by the army, but they also faced pressure by their subjects.[17] As a result, they may have referred dispensable men—such as troublemakers or men who did not contribute much to food production. It follows that the same general testing strategy applies, even if the evidence is potentially less convincing because we have little information on chiefs, and the incentives they face are less clear-cut and probably more political. Once in the army, recruits were treated equally. Having men sent by chiefs included in the analysis improves precision.

V. Empirical Results

We begin by applying the testing strategy outlined in the previous section. We then scrutinize the data in more detail for evidence of referee opportunism. At the end of the section, we subject our results to various robustness checks.

[17] In communication with army officers, some chiefs pointed to the unpopularity of recruitment measures and that they would risk destoolment when meeting their quota.

TABLE 3
REFERRAL AND OBSERVABLE QUALITY

	Height (cm)		Chest Circumference (cm)	
	(1)	(2)	(3)	(4)
Referred by fellow soldier	1.152***	.575*	.490*	.039
	(.428)	(.320)	(.267)	(.191)
Sent by traditional chief	−.977**	.102	−.655	.575
	(.423)	(.429)	(.407)	(.347)
Volunteer	.427	1.109**	−.120	.865**
	(.489)	(.476)	(.445)	(.431)
Year of enlistment fixed effects		Yes		Yes
Age fixed effects (14–23 years)		Yes		Yes
Number of observations	8,295	8,295	8,134	8,134
R^2	.012	.091	.007	.143

Note. Estimator is ordinary least squares. All regressions include a constant. Robust standard errors in parentheses clustered by ethnic group (82 clusters).
* $p < .1$.
** $p < .05$.
*** $p < .01$.

A. Testing the Screening Motive

Our first set of results, presented in table 3, focuses on recruit characteristics observed at the time of recruitment. The dependent variables are height and chest circumference measured at enlistment. In columns 1 and 3 we only include dummies for the method of recruitment (i.e., referred by a fellow soldier, sent by a traditional chief, or volunteered). The omitted category is "no entry"—which refers to direct recruitment by the army, typically through recruitment campaigns. Because health status may be correlated between recruits of the same ethnic group, we cluster standard errors by ethnicity. Results show a strong positive association between referral, height, and chest circumference. In contrast, recruits sent by chiefs tend to be shorter with a smaller chest circumference.

Changes in labor market conditions may be correlated with recruitment methods. More pressure was put on traditional chiefs to send recruits during World War I. The years 1917 and 1918 witnessed the largest buildup of the colonial army to assist in the east African campaign. Some 81% of all soldiers in our data set were recruited during World War I, and 64% in 1917–18 alone. The increased demand for soldiers was not met by referred recruits (fig. 1). Moreover, referrals were more frequently used for young recruits, many of whom were still growing and therefore had a lower height and chest circumference.[18] To correct for this, we reestimate the regressions with year of

[18] In well-nourished populations, most boys attain their final adult height by age 18 (WHO 1995). Under adverse nutritional and health conditions, however, body growth may continue until approx-

enlistment and age fixed effects. Results are shown in columns 2 and 4 of table 3. The referral dummy remains positive but is no longer significant for chest circumference.

A potential source of bias is the former occupation of the new recruit. Some 61% of new recruits list farming as their former occupation. Given the economic context of the time, we expect farmers to come from a poorer background and hence to be shorter than average. If bringers tend to recommend fellow villagers, this could cause a bias. Similar concerns apply to ethnicity and other observables at the time of recruitment.

We reestimate both regressions including the previous occupation information available from army records. This includes dummies for whether the recruit's previous occupation was in farming or in the armed forces (2%). We also include a literate dummy and skill index dummies. Only 7% of recruits are listed as literate. To construct the skill index, we use information on the recruit's occupation before joining the army and rank the occupations from 1 to 5, from least to most complex. This constitutes our skill index.[19]

Results in columns 1 and 3 of table 4 show little change with respect to our main variable of interest. Farmers are significantly shorter than other recruits. Literate recruits tend to be taller, but the coefficient is not significant at the 10% level. Skilled and literate recruits tend to have a smaller chest circumference, a possible consequence of engaging in less strenuous work. In the eyes of army recruiters, their skills may have compensated for perceived strength deficiency.

Next we turn to characteristics that were not observable at recruitment but are subsequently revealed through poor work performance. The two dependent variables of interest are desertion and early dismissal as inefficient or medically unfit or for misconduct. In both cases, the event of interest unfolds over time: the longer a recruit stays in the army, the longer the exposure to the risk of deserting or being dismissed. To account for the length of exposure, we estimate duration models. Results are presented in table 5.[20] Coefficients are reported in the form of hazard ratios: estimates larger than 1 imply a higher risk of desertion or dismissal, and vice versa for estimates smaller than 1. The same regressors are used as in table 3.

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imately age 23. The age groups 14–17 and 18–23 make up 3.4% and 43.2% of our sample; referrals were used in 55% and 36% of the cases, respectively.

[19] The classification follows Armstrong (1972). For occupations falling into the five categories, see table 1. Farmers were coded with a separate dummy variable.

[20] Standard errors are clustered by ethnic group to allow for correlation in residuals. We also estimated duration models stratifying by ethnic group to allow for a different baseline hazard for each ethnic group. Results do not change.

TABLE 4
REFERRAL AND OBSERVABLE QUALITY—WITH CONTROLS

	Height		Chest Circumference	
	(1)	(2)	(3)	(4)
Recruiting channel:				
Referred by fellow soldier	.402*		−.264	
	(.223)		(.162)	
Rank of bringer at the time of referral:				
Low rank (private, lance corporal, corporal)		.199		−.380*
		(.259)		(.194)
High rank (sergeant, company sergeant major, regimental sergeant major, unknown)		.552**		−.179
		(.255)		(.182)
Sent by traditional chief	.093	.097	.407**	.409**
	(.261)	(.261)	(.179)	(.179)
Volunteer	1.093***	1.097***	.826***	.829***
	(.322)	(.322)	(.258)	(.258)
Relatives in the army:				
Recruit has relative in the army (1 = yes)	−.696***	−.706***	.192	.185
	(.254)	(.254)	(.197)	(.197)
Bringer is kin (1 = yes)	−.340	−.252	−.379	−.324
	(.481)	(.484)	(.372)	(.374)
Bringer (yes = 1)	.120	.125	.523	.526
	(.482)	(.480)	(.359)	(.359)
Previous occupation:				
Literate (yes = 1)	.284	.278	−.537**	−.540**
	(.314)	(.314)	(.237)	(.237)
Farmer	−1.266*	−1.279*	−.054	−.057
	(.677)	(.677)	(.306)	(.306)
Armed forces (police, army)	.471	.470	.764*	.761*
	(.540)	(.540)	(.398)	(.398)
Skills in previous occupation fixed effects (from 1 unskilled to 5 skilled)	Yes	Yes	Yes	Yes
Other controls:				
Motor transport unit (yes = 1)	1.601***	1.613***	.541**	.548**
	(.331)	(.331)	(.248)	(.248)
Age fixed effects (14–23 years)	Yes	Yes	Yes	Yes
Year of enlistment fixed effects	Yes	Yes	Yes	Yes
Ethnic group fixed effects	Yes	Yes	Yes	Yes
F-test of equality in coefficients (H0: high rank = low rank; *p*-value)		.169		.288
Number of observations	7,827	7,827	7,682	7,682
R^2	.159	.160	.195	.195

Note. Estimator is ordinary least squares. All regressions include a constant. Robust standard errors in parentheses.
* $p < .1$.
** $p < .05$.
*** $p < .01$.

Results indicate that referred recruits and recruits sent by chiefs are at a much higher risk of desertion or early dismissal. This suggests that referral did not serve a screening purpose. There was a significant drop in desertions during World War I and 1917–18—probably because sanctions were much

TABLE 5
REFERRAL AND UNOBSERVABLE QUALITY

	Desertion			Dismissal		
	(1)	(2)	(3)	(4)	(5)	(6)
Referred by fellow soldier	2.050***	1.556***	1.435***	1.543***	1.179*	1.089
	(5.405)	(4.039)	(3.188)	(4.825)	(1.717)	(.874)
Sent by traditional chief	1.905***	1.734***	1.755***	1.983***	1.787***	1.759***
	(3.680)	(3.635)	(3.820)	(5.794)	(5.303)	(5.568)
Volunteer	1.316	1.190	1.240	1.127	1.008	1.015
	(1.471)	(.923)	(1.179)	(.671)	(.050)	(.092)
Age at enlistment fixed effects (14–23 years)		Yes	Yes		Yes	Yes
Year of enlistment fixed effects		Yes	Yes		Yes	Yes
Year of service fixed effects			Yes			Yes
Number of failures (desertions/ dismissals)	914	914	914	1,333	1,333	1,333

Note. Cox proportional hazard model. Reported coefficients are hazard ratios; *z*-values in parentheses; standard errors clustered by ethnic group. Number of subjects = 7,825.
* $p < .1$.
*** $p < .01$.

harsher. This does not, however, affect our results: as seen in columns 3 and 6 of table 5, results do not change when we include year of service fixed effects.

As we did for table 3, we reestimate the regressions with occupation and skill variables. We expect farmers to be more at risk of desertion, and this is for several reasons. First, farming is a seasonal activity: soldiers may want to return home to assist with the harvest, which conflicts with terms of employment in the army. Second, the soldier may return to the village to take over the family farm from an ailing or deceased parent. This makes us suspect that soldiers with a farming background are more likely to desert. They may also slack in their duties in the hope of being dismissed as inefficient or unfit. Note that skill and occupation are at least partly observable by army recruiters—and what we know is what they put down in their records. If these characteristics predict desertion, this was in principle observable to the employer at the time of recruitment and should therefore have been taken into account at hiring.

When we reestimate the regressions in table 5 with these extra variables, we find that recruits with a farming background had the same higher risk of desertion and dismissal as recruits that had an unskilled occupation before entering the army (reference category). The referral dummy remains positive and significant for desertion, albeit with a smaller coefficient.

Taken together, these results contradict the pure-screening model of referral: referred recruits are at least as good—if not better—as unreferred recruits in terms of observed characteristics such as height and chest circumference but much worse in terms of unobservables, judging by their subsequent work performance.

B. Referee Behavior

The results presented so far raise the possibility of referee opportunism. We now investigate this idea in more detail. We begin by looking for circumstantial evidence of collusion between bringer and recruit, a particularly pernicious form of opportunism. Suppose the bringer and recruit agree to collect the reward and then desert. Given that a percentage of a soldier's salary is only returned at the time of discharge, the financial penalty for deserting increases over time. We would therefore expect collusion to show up in the form of a higher incidence of desertion among refereed recruits shortly after hiring.

To investigate this possibility, we plot in figure 3 the Kaplan-Meyer survival function, that is, the proportion of new recruits who do not desert against the time since the beginning of their contract. We observe a relatively rapid fall in survival rate at the beginning of the contract, consistent with the idea that those who decide they do not like the army leave early. But the figure also shows no difference between referred and unreferred recruits up to 6 months after hiring. It is only after 6 months that we observe a higher desertion rate among referred recruits. We therefore find no prima facie evidence that referee and recruit collude to defraud the army of the bringer's reward.[21] This is, of course, only one extreme form of untruthful refereeing.

In the Montgomery model, high-quality workers are assumed to know other high-quality workers. It is this correlation that makes referral useful for screening new candidates. This correlation, however, implicitly assumes that the referee truthfully reports information he has about the quality of new recruits. We have information on the rank of the bringer at the time of the referral, an index of quality. In the context of the British colonial army over the study period, indigenous soldiers—and hence bringers—occupied only five ranks. Army records show the rank of the bringer at the time of the referral to be private (31% of referred recruits), lance corporal (11%), corporal (9%), sergeant (21%), CSM (16%), or RSM (3%).[22] If the Montgomery model is correct for our data, higher-ranked bringers should bring better recruits. In contrast, if referees behave opportunistically, higher-ranked servicemen may have found it easier to force the hand of army recruiters. In our context, this means getting the bringer's reward even when the proposed recruit is of worse quality. Of course, the army may retaliate against a bringer whose recruits turned out to be unsuitable. It is highly unlikely that the army awarded a

[21] From the data set we could match 726 bringer-recruit pairs. From the 93 recruits who deserted, in only two cases did the bringer also desert. These two bringers deserted 85 days and 161 days earlier than their referred recruit, respectively.

[22] Percentages do not sum to 100 because in some cases the rank is missing or not clear from the record (e.g., *malam*, pay clerk, orderly room clerk, headman).

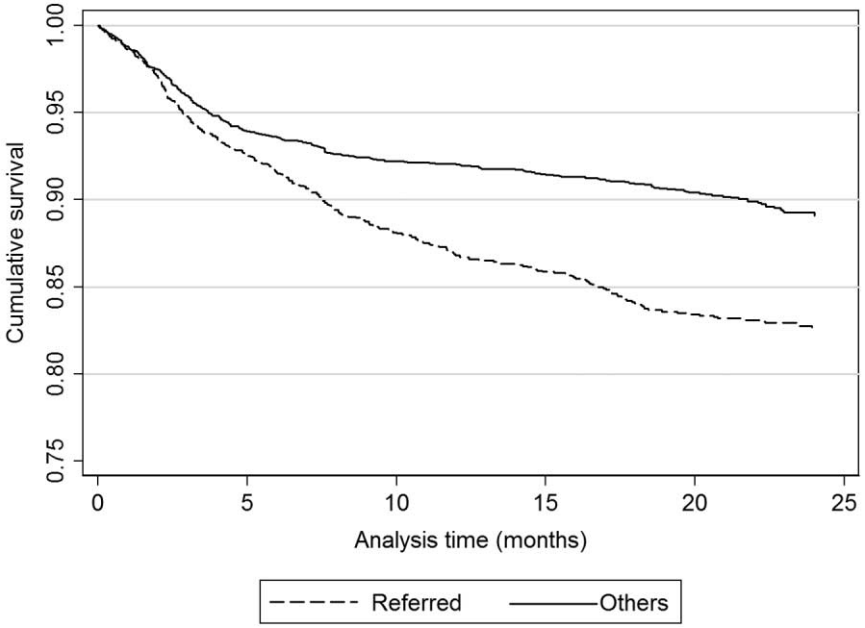

Figure 3. Kaplan-Meyer survival function

reduction in rank, and we have not found a single documented case of this in available sources. Referring unsuitable recruits did not fall into offense categories listed in army regulations that were punishable with reduction. Moreover, for such a punishment, direct intention needs to be proven, which would be difficult. Hence the strongest penalty that can credibly be imposed is delayed promotion. This penalty, however, would have little or no effect on sergeants and CSMs given that, in the colonial army, these were the highest ranks African soldiers could hope to reach. If referees behave opportunistically, we therefore expect higher-ranked bringers to bring recruits of lower (not higher) unobserved quality—in reaction to which the army may insist on better observable characteristics.

The attestation papers also report whether the new recruit was a relative of the bringer. This concerns 3% of all recruits but 8% of all those referred. We expect referees to have better information about the unobserved quality of their relatives—and hence to be able to recommend among their relatives those who are better suited for army work. Bringers may, however, be pressured by relatives to provide a recommendation (as in Beaman and Magruder 2012)—or, if they are facing a lot of pressure from officers to bring recruits, they may pressure relatives to join. In either of these cases, we expect the quality of new recruits to be worse when they are recommended by a relative.

By the same reasoning, recruit quality should be lower for those with relatives in the army who may indirectly have favored their candidacy or encouraged them to join. Some 15% of recruits are recorded as having a relative in the army. This is a high proportion, so we expect the term "relative" to have been interpreted rather loosely (e.g., as synonymous for kinship).

We test these predictions by adding dummies for whether the bringer is kin and for whether the recruit has relatives in the army. Results for height and chest circumference are presented in table 4. From columns 1 and 3 we see that recruits with family ties in the army are hired despite being of significantly shorter height. New recruits explicitly referred by a relative are shorter and have a smaller chest circumference—consistent with favoritism—but the coefficient is not statistically significant. In columns 2 and 4 we add dummies for high- and low-rank bringer. We find that recruits referred by higher-ranked soldiers tend to be taller and have a larger chest circumference than recruits referred by low-ranked soldiers, although the difference in the ranks is not statistically significant.

Next we turn to unobserved characteristics *d*. In table 6 we find some evidence of favoritism: recruits with family ties in the army were hired even though they are more likely to be dismissed. This is a general observation and is not limited to cases in which a kin was listed as bringer, and it stands in contrast to the findings reported by Costa and Kahn (2003) for US Civil War units. In columns 2 and 4 of table 6 we see that recruits brought by a high-rank bringer are more likely to desert than those brought by a lower-ranked soldier. We do not find a similar pattern for dismissals however.

The desertion results from table 6 indicate that higher-ranked referees bring recruits of lower unobserved quality. This finding is consistent with the idea that referees behave opportunistically and that a higher-rank bringer can no longer be punished by being denied promotion. This can also explain our earlier observation from table 4 that recruits referred by higher-ranked soldiers have higher observable characteristics—army recruiters may be compensating for lower anticipated unobserved quality.

C. Desertion and Deterrence

To summarize, we have found some evidence consistent with referee opportunism but no evidence that worker referral improves screening on unobservable characteristics. We now investigate whether soldier referral may nevertheless serve a monitoring purpose. Given that bringer and recruit are rarely assigned to the same unit, the bringer is unlikely to play a role in day-to-day monitoring. The bringer may nevertheless help locate a deserting recruit, and thus may help deter desertion. We have already seen that, on average, this

TABLE 6

REFERRAL AND UNOBSERVABLE QUALITY—WITH CONTROLS

	Desertion		Dismissal	
	(1)	(2)	(3)	(4)
Recruiting channel:				
Referred by fellow soldier	1.268**		.973	
	(2.188)		(−.300)	
Rank of bringer at the time of referral:				
Low rank (private, lance corporal, corporal)		1.101		.978
		(.748)		(−.210)
High rank (sergeant, company sergeant major, regimental sergeant major, unknown)		1.391***		.969
		(2.862)		(−.312)
Sent by traditional chief	1.489***	1.496***	1.391***	1.390***
	(3.266)	(3.301)	(3.099)	(3.094)
Volunteer	1.044	1.052	1.087	1.087
	(.245)	(.287)	(.525)	(.523)
Bringer (yes = 1)	1.200	1.205	.883	.883
	(.695)	(.710)	(−.629)	(−.631)
Relatives in the army:				
Recruit has relative in the army (yes = 1)	1.043	1.028	1.186**	1.187**
	(.367)	(.244)	(2.006)	(2.010)
Bringer is kin (yes = 1)	.973	1.042	1.342**	1.339*
	(−.141)	(.216)	(1.995)	(1.959)
Previous occupation:				
Literate (yes = 1)	.807	.803	.957	.957
	(−1.243)	(−1.271)	(−.304)	(−.302)
Farmer	1.114	1.112	2.245	2.245
	(.243)	(.239)	(1.450)	(1.451)
Armed forces (police, army)	.922	.904	1.089	1.089
	(−.250)	(−.306)	(.357)	(.358)
Skills in previous occupation fixed effects	Yes	Yes	Yes	Yes
Other controls:				
Motor transport unit (yes = 1)	.679*	.688*	.823	.823
	(−1.766)	(−1.708)	(−.995)	(−.997)
Height (cm)	1.006	1.006	1.004	1.004
	(1.125)	(1.114)	(.784)	(.786)
Chest circumference (cm)	1.010	1.010	.984**	.984**
	(1.374)	(1.355)	(−2.368)	(−2.364)
Year of enlistment fixed effects	Yes	Yes	Yes	Yes
Year of service fixed effects	Yes	Yes	Yes	Yes
Age at enlistment fixed effects (14–23 years)	Yes	Yes	Yes	Yes
Ethnic group fixed effects	Yes	Yes	Yes	Yes
χ^2 test of equality in hazards (H0: high rank = low rank; *p*-value)		.03		.92
Number of failures (desertions/dismissals)	844	844	1,188	1,188

Note. Cox proportional hazard model. Reported coefficients are hazard ratios; *z*-values in parentheses; standard errors clustered by ethnic group. Number of subjects = 7,141.

* $p < .1$.

** $p < .05$.

*** $p < .01$.

deterrence effect is not so strong that it reduces desertion among referred recruits. But this may be because deterrence only affects a subgroup of recruits. The purpose of this section is to investigate this possibility.

We begin by establishing that the risk of desertion increases with distance between a recruit's place of attestation and his place of origin. The rationale is that it is easier for the army to find and bring back those who run away to a nearby village. Regression results, not shown here to save space, indicate that, for Ghanaian recruits, the likelihood of desertion increases with the distance between place of attestation and place of origin. This finding does not carry over to the full sample, however. This is hardly surprising given that men returning to their villages outside Ghana (e.g., in French West Africa or Liberia) were beyond British jurisdiction.

Findings are reported in table 7 using only recruits of Ghanaian origin. Columns 1 and 4 report coefficient estimates without distance. These are given for comparison purposes only.[23] In columns 2 and 5 we introduce distance k and an interaction term between k and referral. We find that distance is associated with a higher likelihood of desertion and dismissal, but this association is more than neutralized for referred recruits. This is consistent with the idea that distant recruits that are referred can be located more easily and hence, other things being equal, are less likely to desert than unreferred distant recruits. Also note the sharp increase in the hazard ratio of the referral dummy after controlling for the desertion deterrence effect of referrals with respect to distance: once we control for the deterrence effect of referral, the effect of referral on desertion is much larger.

Next we introduce an interaction term $k\Delta a$, where Δa is proxied by 1917–18 when the war intensified and desertion was punished more severely. We also interact $k\Delta a$ with referral. Results are presented in table 7 columns 3 and 6. For desertion we find, as expected, that the disincentive effect of proximity is stronger when desertion is punished more severely. This is apparent from observing that the coefficient of $k\Delta a$ is larger than 1 and strongly significant. The direct effect of Δa itself, however, is a strong reduction in the likelihood of desertion, as evidenced by a coefficient of the "serving in 1917–18" dummy far below 1. The interaction term between referral and $k\Delta a$ shows, as anticipated, an odds ratio well below 1 for desertion.

Turning to dismissal, we find that the likelihood of dismissal increases with the distance between military base and place of origin. This suggests that the army may have dismissed recruits who perhaps could not be prevented from

[23] The referral coefficient is nonsignificant. Note, however, that the hazard ratio is only slightly smaller than in table 6, col. 1, whereas sample size and number of desertions is much reduced.

TABLE 7
REFERRAL AND UNOBSERVABLE QUALITY—WITH DISTANCE (GHANAIAN RECRUITS ONLY)

	Desertion			Dismissal		
	(1)	(2)	(3)	(4)	(5)	(6)
Referred by fellow soldier	1.189	1.929***	1.672*	1.060	1.803**	1.851**
	(.925)	(2.726)	(1.934)	(.339)	(2.556)	(2.541)
Distance (in 100 km) between place of birth and:						
Military base (place of certificate)		1.129**	.958		1.148***	1.171**
		(2.325)	(−.427)		(3.290)	(2.339)
Military base × referred		.798***	.933		.830***	.817***
		(−3.028)	(−.615)		(−3.273)	(−2.729)
Military base × serving in 1917–18			1.225**			.975
			(1.990)			(−.353)
Military base × serving in 1917–18 × referred			.833			1.012
			(−1.396)			(.153)
Number of failures (desertions/dismissals)	390	390	390	539	539	539

Note. Cox proportional hazard model. Reported coefficients are hazard ratios; robust *z*-values in parentheses. Controls as in table 6, col. 1; coefficient in col. 1 differs from table 6, col. 1, due to restriction of the sample to Ghanaian recruits only. Number of subjects = 3,445.
* $p < .1$.
** $p < .05$.
*** $p < .01$.

deserting. Again we find that this relationship does not apply to referred soldiers. In contrast to desertion, we find no evidence of a 1917–18 effect: the coefficients of both $k\Delta a$ and its interaction with referral are not statistically significant. This is not surprising: dismissal is initiated by the army, and during the war all available soldiers were needed, even those who were less efficient or less fit. To summarize, the results are consistent with a deterrence effect of referral on desertion but not on dismissal.

D. Further Robustness Analysis

Before concluding, we investigate whether the patterns documented here are robust or whether they are driven by specific observations. First, we examine whether our results are driven by a small number of very active referees. To this effect, we exclude recruits who were brought by recruiters who referred more than nine new recruits in total, and we reestimate the regressions presented in tables 4 and 6. The total number of referred recruits drops by 30%. Regression results on height and chest circumference, not reported here to save space, are very similar to those reported earlier. Regression results on desertion and dismissal confirm earlier results and indicate a higher likelihood of desertion and dismissal among referred recruits. We also find that recruits brought by higher-ranked soldiers are more likely to desert. The effect, how-

ever, is less strong than before, possibly because those who bring many recruits hold higher ranks.

We also check whether the assumption about termination date affects our findings. Because missing information on date of discharge is nonrandom, we reestimate the duration models with a selection correction using the method proposed by Boehmke, Morey, and Shannon (2006). Results are confirmed.[24]

To conclude this section, we investigate whether the quality of referred recruits varies with the average incidence of desertion and dismissal before the date of enlistment of the new recruit. The idea is that, if desertion is relatively common, the potential for opportunistic referral is higher since, presumably, penalties for bringing an unsuitable recruit will be lower when the quality of new recruits is generally poor.

It is reasonable to assume that bringers had some idea of desertion rates prevailing at the time of referral. They may therefore respond to the deterioration of the selection process by bringing less suitable recruits. We test this idea by including as additional regressor on the desertion rate in the 12 months preceding the recruit's enlistment, and we interact this regressor with the referral dummy. The total number of new recruits is also included as additional control, to avoid spurious inference due to changes in recruitment levels. Results, shown in table 8, indicate that referred recruits are more likely to subsequently desert when the incidence of desertion was high in the period immediately preceding their recruitment. Although it is difficult to assess statistical significance due to multicollinearity, the result is again consistent with referee opportunism.[25] There is no such pattern for dismissals.

E. Discussion

We have organized our testing strategy primarily focusing on the screening role potentially played by referral. Kugler (2003) has proposed an alternative model in which employee referral is used as a way to save on monitoring costs and to elicit effort from referred employees. We have already discussed this issue briefly in Section III, but we revisit it in more detail here.

For the monitoring motive to be relevant for our context, the referred soldier should be placed under the direct or indirect supervision of the referee so that his effort can be monitored. This simple observation could in principle serve as a basis for distinguishing between the monitoring and screening motives for the army using worker referral.

[24] For details, see online app. C.

[25] The correlation between the referral dummy and its interaction with the desertion rate in the past 12 months is very high.

TABLE 8
REFERRAL AND UNOBSERVABLE QUALITY—WITH INCIDENCE OF DESERTION/DISMISSAL BEFORE RECRUITMENT

	Desertion		Dismissal	
	(1)	(2)	(3)	(4)
Referral by fellow soldier	.967		.935	
	(−.105)		(−.424)	
Incidence of desertion and dismissal:				
Desertion rate in the 12 months before enlistment	.975	.977		
	(−.798)	(−.936)		
Desertion rate in the last 12 months × referred	1.034	1.030**		
	(.885)	(2.343)		
Dismissal rate in the 12 months before enlistment			.987	.990
			(−.768)	(−.654)
Dismissal rate in the last 12 months × referred			1.006	1.001
			(.438)	(.169)
Time at risk 12 months before enlistment	1.000	1.000	1.000	1.000
	(1.371)	(1.380)	(−1.068)	(−1.139)
Number of failures (desertions/dismissals)	844	844	1,188	1,188

Note. Cox proportional hazard model. Reported coefficients are hazard ratios; robust *z*-values in parentheses. Controls as in table 6, col. 1. Number of subjects = 7,141.
** $p < .05$.

Although we have no information on the unit to which a new recruit was sent after his 6 months of training, the limited information available suggests that monitoring by the bringer is unlikely to have been important. We first note that, if the army intended bringers to monitor the new recruits they brought in, we would expect a fair amount of ethnic homogeneity within army units, given that bringers tend to recommend people from the same origin. We find no evidence of this. Army units were ethnically diverse. The lingua francas used in the colonial army were Hausa and English—neither of which is indigenous to Ghana (Killingray 1982).[26] On the basis of information available on the unit and the ethnicity of bringers, we calculated the Herfindahl index of eight army units. The weighted average index is 0.08, which means that if we had taken two soldiers at random from the same unit, they only had an 8% chance of being from the same ethnic group.

We also check whether bringers tend to come from training units where they could have monitored new recruits during their 6-month training period. From available data of 1,107 referrals, only 15 were from soldiers in training units. This compares to a much larger number of referrals (270) from soldiers in units organizationally separated from the GCR and who thus could not

[26] We know of one "Ashanti Company." Ashanti soldiers disliked following orders from officers of northern origin, whom they regarded as of lower status given that slaves were historically drawn from those ethnicities (Killingray 1982).

have monitored new recruits at all.[27] We reestimate tables 4 and 6 with an additional dummy if the bringer no longer was in the army at the time of recruitment. Results, not reported here to save space, are either not significant or not robust across specifications—possibly due to small sample size.

We conclude that there is no evidence in favor of the day-to-day monitoring hypothesis, apart from the evidence we have reported earlier that the bringer may have played a role in helping locate deserting soldiers. Taken together, the evidence indicates that, if the army was hoping to enlist the help of the referee to monitor and incentivize new recruits, this objective was not achieved: if anything, referred recruits were more likely to desert or be dismissed because of low productivity (e.g., misconduct or inefficiency). These findings should thus be construed as rejecting both the pure-monitoring and pure-screening models of employee referral.

This leaves open the question of why the colonial army resorted to employee referral—and paid soldiers for bringing new recruits. The most likely explanation is the need to economize on recruitment costs. During our study period, the colonial army used a variety of methods to bring in new recruits. The method that brought the best recruits was direct recruitment in villages. Given low population densities, paucity of roads, and lack of transportation at the time, this must have been expensive. The colonial administration was not willing to spend much for the army and kept the peacetime strength of the Ghanaian army small relative to the vast territory that it had to secure and protect. Asking traditional chiefs as well as enlisted soldiers to send in new recruits must have saved on recruitment costs, even if it lowered the reliability of recruits. Furthermore, recruits brought by fellow soldiers were on average better than those sent by chiefs, and army recruiters were in a position to compensate somewhat for their lower unobserved quality by insisting on better observable characteristics such as height and chest circumference. These observations militate in favor of a simple transactions cost explanation for employee referral by the colonial army.

This raises the issue of the generalizability of our results. Although the size of the Ghanaian colonial army was determined by political considerations, recruitment methods were probably chosen so as to minimize costs. The colonial army faced difficulties attracting recruits from alternative employment in farming, mining, and the informal sector. In an environment in which em-

[27] One hundred twelve bringers were in the reserve, in which servicemen were organized after completion of regular military service, and 158 bringers were in the Northern Territories Constabulary, an armed police unit based in northern Ghana.

ployers compete for workers, the cost savings should go to workers. Montgomery's model allows employers to offer higher start-up wages to workers hired through employee referral. This is not what happened in the army in Ghana: new recruits were offered a standard contract that was identical irrespective of recruitment method. Whether Montgomery's assumption is realistic thus depends on the legal context.

In many developed economies, paying a new worker more because he or she was recommended by an existing employee would be considered discriminatory—and banned by law. An alternative competitive equilibrium, which is not considered by Montgomery but could be fitted in the same framework, gives the cost savings to the bringer, either in cash—as the colonial army did—or in kind through perks or promotion. The results presented here show that rewarding the bringer generates perverse incentive effects that, in our case, were strong enough to undo whatever screening and monitoring benefits employee referral is supposed to convey.

VI. Conclusion

Using information compiled from army personnel records, we tested whether the referral system in place in the British colonial army in Ghana served to better screen recruits for characteristics unobserved at hiring. We found that the referral system in place did not lead to a selection of recruits with high unobservable quality: referred recruits were taller and had a larger chest circumference—two observable qualities recorded by army recruiters at the time of hiring—but were significantly more likely to desert and to be discharged as medically unfit or inefficient or for misconduct. These findings are difficult to reconcile with a screening model of referral. They are also inconsistent with the idea, proposed, for instance, by Kugler (2003), that employers resort to employee referral with the understanding that referees monitor new recruits. We do, however, find some evidence that referral lowers the likelihood of desertion for those recruits posted far from their place of origin, suggesting that referral played some useful role in locating and bringing back deserters. This finding can be interpreted as a form of monitoring benefit from referral.

We find some evidence to support the hypothesis of referee opportunism. In particular, the unobserved quality of new recruits is worse when the bringer had reached a rank with fewer promotion prospects. We also find that the unobservable quality of referred recruits falls more at times when the proportion of bad recruits is high. Both results are suggestive of opportunistic behavior on the part of referees. We do not, however, find evidence of collusion between bringer and recruit to defraud the army, and we only find

limited evidence of nepotism insofar as new recruits with a kin in the army were accepted even though their lower-than-average height was ex ante observable and they had a higher risk of dismissal.

These results are consistent with a model of opportunistic referral in which army recruiters are aware of the incentive problem and seek to compensate the lower unobservables of referred recruits with higher observables. Referee opportunism begs the question as to why an employer would rely on the practice if it knew that it yielded lower-quality workers. In the case of the Gold Coast Regiment, referrals by fellow soldiers did not produce the worst results: recruits sent by traditional chiefs are worse in all dimensions. The financial cost of using employee referrals may have been lower compared to other recruitment channels, especially recruitment campaigns in the villages. The political cost was also lower since referred recruits could be classified as having volunteered (Killingray 1982). This can explain why the colonial army made use of employee referrals in spite of the lack of evidence that it improved worker screening or served more than a subsidiary monitoring role.

Although our findings are specific to an employer and moment in time, they cast doubt on the idea that employee referral always serves a useful screening or monitoring role. Our findings are more in line with those of Bentolila et al. (2010), who find that referred workers receive lower wages but spend less time unemployed, and especially with those of Beaman and Magruder (2012), who, in an experimental setting, find direct evidence of referee opportunism.

The situation of the colonial army in the early twentieth century is not too dissimilar from the situation of present-day employers of indigenous unskilled workers newly arrived in Africa—such as Chinese and other Asian investors in mining and plantations. Like the Ghanaian colonial army, these employers wish to identify loyal and disciplined workers. Recent press suggests that they do not always succeed. Our article outlines the limits of the usefulness of using worker referral in this context.

Appendix

Identification

Here, we present a more detailed and theoretically based motivation of our testing strategy. The focus is on identification. Consider an employer facing a large pool of potential recruits with unknown employer-specific productivity $\widetilde{q}$ with probability distribution function $g(q)$. For exogenous reasons (e.g., considerations of fairness, loyalty, and political legitimacy), we assume that

this employer must offer all new recruits the same wage and allocate them to the same undifferentiated task.

The employer wishes to select the recruits with the highest productivity. If productivity is observable, the employer simply selects all recruits with productivity above a threshold $q_{\min}$. Thus, $q_{\min}$ is the productivity of the marginal worker who is hired. The average productivity of hired workers is

$$E[\tilde{q} \mid q \geq q_{\min}] = \int_{q_{\min}}^{\infty} qf(q)\, dq;$$

that is, it is the average of the truncated distribution of $\tilde{q}$ above $q_{\min}$. When the employer needs to rapidly expand its workforce, it needs to set a lower threshold $q'_{\min} < q_{\min}$. It follows that

$$E[\tilde{q} \mid q \geq q_{\min}] > E[\tilde{q} \mid q \geq q'_{\min}].$$

When the pool of recruits changes over time, the threshold must also adjust so that the employer can find the new recruits it needs.

Now let productivity be an increasing function of two (sets of) variables h and d such that $q = f(h, d)$ with $\partial^2 q/\partial h \partial d \leq 0$ (i.e., h and d are substitutes). Variable h is observable to the employer at the time of hiring, d is not. In addition, the employer observes a signal θ that is informative about d—and thus about productivity. Without loss of generality, let the signal be defined such that $E[d]$ is increasing in θ. Given this, the optimal policy for the employer is to set a signal threshold locus $\theta_{\min}(h)$ below which recruits are not hired. Furthermore, since h and d are substitutes, $\theta_{\min}(h)$ is a decreasing function of observable characteristic h. Put differently, the employer sets a higher minimum signal to recruit workers with a lower h—and vice versa.

The main difference with the full observability model is that now the employer occasionally hires workers whose realized productivity is below what is necessary for the job. Given this, we expect to observe some workers either to defect because the work is too arduous or to be dismissed by the employer for poor performance. We assume that this is costly for the employer.

As in the full observability case, hiring more recruits requires lowering the threshold locus to, say, $\theta'_{\min}(h)$. It is easy to see that we have

$$E[\tilde{q} \mid h,\ \theta \geq \theta_{\min}(h)] > E[\tilde{q} \mid h,\ \theta \geq \theta'_{\min}(h)];$$

that is, lowering the threshold results in lower average productivity. It follows that if the employer rapidly expands recruitment, we expect a subsequent increase in dismissals and defections.

Screening through Referral

We now introduce the possibility of remunerated worker referral. Let $R = 1$ if a recruit is referred and $R = 0$ otherwise. We now assume that R contains additional information that helps predict d (i.e., that $d = d(\theta, R)$). At the margin, the employer is willing to pay reward τ to a referee for a recruit with characteristics $(h_{\text{ref}}, \theta_{\text{ref}})$ if

$$\begin{aligned} &E[q(h,\ d(\theta,\ R)) \mid h_{\text{ref}},\ \theta_{\text{ref}},\ R = 1] - \tau \\ &\geq E[q(h,\ d(\theta,\ R)) \mid h_{\text{ref}},\ \theta_{\text{ref}},\ R = 0], \\ &\qquad \text{which implies} \\ &E[q(h,\ d(\theta,\ R)) \mid h_{\text{ref}},\ \theta_{\text{ref}},\ R = 1] \\ &> E[q(h,\ d(\theta,\ R)) \mid h_{\text{ref}},\ \theta_{\text{ref}},\ R = 0]. \end{aligned} \tag{A1}$$

Any referred job applicant who does not satisfy this requirement is not hired by the employer—and the reward τ is not paid. It immediately follows that $R = 1$ must predict better performance q. In other words, the fact that a job applicant is referred must be interpreted by the employer as a positive informative signal for the employer to agree to remunerate referral. This could naturally arise because, as assumed by Montgomery (1991), "birds of a feather flock together": referring workers who have demonstrated their productivity by not defecting or being dismissed know potential recruits who, on average, also have better productivity—and $d(\theta,\ R = 1) \geq d(\theta,\ R = 0)$. Alternatively, it could be because referees exert effort in identifying suitable recruits in order to obtain the reward τ. Whatever the reason, if being referred is not informative, the employer should not offer a reward. If referral predicts lower performance, we should not observe it (i.e., job applicants would apply without providing a referral).

Equation (A1) is the basis for our testing strategy: if referral conveys positive information about unobserved quality over and above h_{ref} and θ_{ref} (which in this model it should, since it is remunerated), then the average performance of referred workers should be higher than that of unreferred workers conditional on the information $(h_{\text{ref}}, \theta_{\text{ref}})$ observable to the employer at the time of recruitment.

To demonstrate why we need to control for h_{ref} and θ_{ref} for our test to be identified, imagine that we do not. If referees on average know better workers, we expect $E[h \mid R = 1] \geq E[h \mid R = 0]$ and $E[\theta \mid R = 1] \geq E[\theta \mid R = 0]$ (i.e.,

referred workers should be better than unreferred ones on observables). It follows that, in this case, it is possible that

$$E[q(h,\ d(\theta,\ R))\mid R=1] > E[q(h,\ d(\theta,\ R))\mid R=0],$$

even though

$$\begin{aligned} &E[q(h,\ d(\theta,\ R))\mid h_{\text{ref}},\ \theta_{\text{ref}},\ R=1]-\tau \\ &< E[q(h,\ d(\theta,\ R))\mid h_{\text{ref}},\ \theta_{\text{ref}},\ R=0]. \end{aligned}$$

Put differently, if we do not control for the determinants of productivity that the employer observes at recruitment, we could erroneously ascribe an informational benefit to referral.

The reverse is more likely in equilibrium. For all recruits who are strictly above the threshold $(h,\ \theta_{\min}(h))$, the employer does not need the additional information provided by referral—and thus has no reason to remunerate referees for it. Referral is only useful to the employer for marginal workers, that is, for workers who would not be employed if there were not referred. It follows that the set of workers who are recruited as a result of referral should, in equilibrium, have lower h and θ than workers hired without referral. In this case, not conditioning on h and θ would result in

$$E[q(h,\ d(\theta,\ R))\mid R=1] < E[q(h,\ d(\theta,\ R))\mid R=0].$$

We would erroneously conclude that referral is correlated with lower productivity when in fact, among marginal recruits with low h and θ, it is correlated positively with it.

Economizing on Recruitment Costs

The situation is different if referral is not used as a screening device but as a way of economizing on recruitment costs. To capture this idea, let c be the marginal cost to the employer of identifying an additional recruit on its own. In the context of our study, this typically means fielding a recruitment campaign in the countryside at a time when the population is scarce and dispersed and transportation is difficult. Let $\widetilde{r}$ be the cost of identifying an additional recruit for workers who are already employed. Assume that for some workers $r < c$: these workers can identify employable recruits among their acquaintances and kinsmen.

Now suppose that the employer offers a reward $\tau < c$ for referring a suitable worker, that is, for bringing a recruit with observables (h, θ) above the thresh-

old $(h, \theta_{\min}(h))$. Workers whose search cost $r < \tau$ have an incentive to refer, and the employer has an incentive to pay the reward for referral. In this case referral conveys no information advantage.

It is also possible that referred workers are on average worse than the recruits the employer identifies by incurring the recruitment cost c. This could arise because of opportunistic or favoritist referral or for other reasons. In this case, referred workers are worse than normal recruits conditional on observables; that is, we have

$$E[q(h, d(\theta, R)) \mid h_{\text{ref}}, \theta_{\text{ref}}, R = 1] < E[q(h, d(\theta, R)) \mid h_{\text{ref}}, \theta_{\text{ref}}, R = 0]$$

because $d(\theta, R = 1) < d(\theta, R = 0)$. If the employer anticipates this, a higher threshold $(h, \theta^{\text{ref}}_{\min}(h))$ should be applied to referred workers to compensate. For instance, the employer may only hire referred applicants who have a particularly high h. In this case we will observe that, conditional on h and θ, the performance of referred recruits will be worse than unreferred recruits, but referred recruits will have more desirable observed characteristics on average.

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